Unusually Large Hail.

SOME very large hailstones fell here about 3.30 p.m. on Sunday, June 2, during a short but sharp thunder-storm. Most of them were ellipsoidal in outline: some were mammillated, and some were evidently compound, formed of several hailstones partially fused together. Ten picked up at random as being fairly large ones measured from $\frac{7}{8}$ to $1\frac{3}{4}$ inches ($\frac{7}{8}$, $\frac{7}{8}$, 1, $1\frac{1}{8}$, $1\frac{1}{8}$, $1\frac{1}{8}$, $1\frac{1}{2}$, $1\frac{1}{2}$, $1\frac{3}{4}$, $1\frac{3}{4}$ inches) in greatest diameter. Many of them were formed of four, five, or six concentric layers, which were alternately clear and snow white. Some of these hailstones lying on the grass took more than an hour and a half to melt away (temp. 65° F.)

Mr. I. C. Thompson has just drawn my attention to the residue which they leave when melted on a clean glass slide. This, when examined under a high power of the microscope, is found to contain, along with inorganic particles, a number of minute plant spores. W. A. HERDMAN.

University College, Liverpool, June 2.

The Muybridge Photographs.

ALLOW me to state, in order to save correspondence due to the omission of a publisher's name in connection with the Muybridge photographs (NATURE, May 23, p. 78), that they may be seen and ordered of Mr. Muybridge, at 38 Craven Street, Strand, London. E. RAY LANKESTER.

University College, London.

THE VICES OF OUR SCIENTIFIC EDUCATION.[1]

THE subject which I desire to bring to the notice of the Association to-day must necessarily remind you of the attack which was recently made on our competitive examination system in the *Nineteenth Century*.

In some respects the writers of the article, or articles, in the *Nineteenth Century*, appear to me to have right on their side when they object to the existing state of our competitive system.

They begin by complaining against the dangerous mental pressure, and the resulting physical mischief, which accompany the working of nearly all parts of our present educational system. This complaint seems to me to be just.

The fact is, in my opinion, that nearly all our examinations are much too difficult—too much beyond the mental and physical abilities of the examined.

Let me take, as a supreme instance, the examination for the Mathematical Tripos at Cambridge. Ever since I have known anything of this examination, I have wondered how it can be possible that young men, three years after leaving school, can successfully grapple with problems, a great number of which are of transcendent difficulty, in such an immense range of mathematical and physical subjects as this Tripos contains. Indeed, when we are admitted behind the scenes by reading the solutions of these problems by those who have set them, our wonder is increased; for we find frequently that the discussion of a single problem occupies four, five, or six (and sometimes more) pages of small print. Such problems have a very great value for the student who has plenty of time to consider them in the solitude of his study; but I should think that the attempt to attain the amount of knowledge and adroitness necessary to deal with them on the spur of the moment in the Senate House must often produce mental and physical injury. Are we really to believe that a young man of twenty-one or twenty-three has made himself master of nearly everything given to the scientific world by Newton, Laplace, Gauss, Jacobi, Helmholtz, Cayley, Thomson, and Clerk Maxwell?

The desire to place before a student a standard which (I suppose I am right in assuming) he can never reach is, of course, quite defensible; and it is one which appears in every educational competition. But it is not in all cases carried out with a regard for rightness of method. For, this desire to be always in advance of the student leads some examining bodies to hurry him through a large number of subjects in a short time. It is, I think, a marked characteristic of our very modern method that we require half a dozen branches of mathematics and physics to be got through in a time which, say, twenty years ago, would have been devoted to the study of two or three. Is there, for example, nearly so much time now devoted to the study of pure Geometry as there was then? Is Trigonometry so thoroughly and leisurely studied now in the schools?

With the rage which now exists for rushing students through elementary mathematics in order that they may in the shortest possible time reach physics, both experimental and mathematical, the necessary foundations of scientific knowledge are seldom properly laid. Boys who ought to be learning skill in Algebraical manipulation, in assimilating Trigonometrical formulæ, and in applying them to various problems of Mensuration, are, I find, endeavouring to limp through Statics, Hydrostatics, and Kinetics. When to these comparatively advanced subjects we add some Chemistry, the phenomena (at least) of Heat, Optics, Sound, Electricity, and Magnetism—to say nothing of languages—the result is inevitable that the less showy subjects of elementary pure Mathematics must be insufficiently studied—must, in fact, be merely skimmed.

There is no subject in which the result of this over-haste is so easily recognized as Trigonometry; for, at the outset, the student's work in this branch must largely consist in committing to memory a number of formulæ, and nothing but long-continued practice in application will fix them in the mind. Hence, as the necessary time must be given to several other subjects, I find very many students exceedingly slow in repeating, and even in recognizing, some of the most elementary and frequently useful formulæ in Trigonometry. Hence also a large portion of the knowledge brought out in competitive examinations consists of what is called "Cram," and it is, therefore, customary to heap odium on the "Crammers." I do not think, however, that the fault rests with the Crammers, who do merely what they are invited to do by educational authorities.

I shall take as an instance of the excessive haste with which students are pushed on through various branches of Science the Matriculation Examination of London University; and what I say with reference thereto is the result of a present experience which I have in assisting a young relative in his reading for this examination. In last year's Regulations for Matriculation you will find that the course of Mathematics consists of Arithmetic, Algebra as far as easy quadratic equations with questions involving their use, Geometry to the extent of the first four books of Euclid, with simple deductions; and in Mechanics the requisites are "elementary notions as to Velocity, Acceleration, Force, Mass, Momentum, Work, and Energy, Composition and Resolution of Velocities, Accelerations, and Forces in one plane. Moments and Couples in one plane. Centre of Gravity, or Mass-centre. Transmission of Pressure in Liquids; variation with depth of the pressure due to weight of liquids. Specific Gravity and modes of determining it. Pressure of gases, and laws relating thereto. Atmospheric pressure. Common instruments and apparatus whose action depends upon the pressure of liquids, or of the atmosphere, or both." In addition to this, the candidate must take up either Chemistry, or Heat and Light, or Electricity and Magnetism.

Now you will observe particularly two things about this prescribed course. Firstly, the candidate is not supposed to have any knowledge of the fifth and sixth books of Euclid, and therefore no knowledge of the propositions relating to the ratios of linear or other magnitudes; and, secondly, that all knowledge of Trigonometry is excluded.

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[1] A Paper read before the Association for the Improvement of Geometrical Teaching, January 19, 1889, by Prof. Minchin, President.

What boy with this slender knowledge of pure Mathematics is likely to have a correct notion of the nature of Acceleration? And, without knowing the meaning of a sine or a tangent, to what can his knowledge of the Composition and Resolution of Forces and of the method of taking Moments amount?

A course of reading in Mechanics and Hydrostatics, specially constructed so as to avoid Trigonometry and the sixth book of Euclid, is a curious object to contemplate. It reminds one of a number of large boulders thrown out into a stream, with painfully and dangerously wide intervals between them, their tops barely above the water, to enable a man to get across by a series of courageous jumps; and, as in the case of such a traveller, not all the caution nor the acrobatic deftness of balancing on the point of one foot which he possesses will prevent him from tumbling off his flimsy and rickety supports, so in the case of the student for whom knowledge has been packed up into a number of scientific boluses, it is impossible to avoid the acquisition of erroneous notions and fallacious rule-of-thumb principles, which, assimilated thus early, have a strong tendency to remain rooted in the mind.

In the case of my young friend I found it quite impossible to impart anything that could with propriety be called a knowledge of Mechanics without the aid of Trigonometry. In the Composition and Resolution of Forces a few questions made specially to order, which usually depended on the facts that the sine and cosine of 45° are equal, and that the sine of 30° is $\frac{1}{2}$—although, of course, the mention of a sine or cosine was inadmissible—exhausted the field; and the same restrictions were imposed on the treatment of Moments, so that I was obliged to abandon the task, and to insist on a small knowledge of Trigonometry and the sixth book.

In such a cramped and stunted knowledge there is nothing of spontaneity, nothing of power, but much of danger. It is far better to give no encouragement to it, to defer the attempt to study the elements of mathematical physics until the old and well-recognized branches of elementary pure mathematics have been studied with some thoroughness.

The want of thoroughness which seems to me to be so prominently characteristic of our works on mathematical physics sometimes exhibits itself, quite unconsciously on an author's part, in a ludicrous manner. It may, perhaps, be best described as "Calculus dodging." For some curious reason, which I have never discovered, it has been generally assumed that a student can possess a very extensive knowledge of the results and principles of Dynamics—of the composition and resolution of forces and couples in three dimensions, of the principle of work and energy, of the nature and properties of tubes of force, Potential, &c.—without any knowledge of the Differential or Integral Calculus. This is, surely, a piece of self-deception. The processes of differentiation and of elementary integration are not difficult of acquirement, and it seems to me that they ought to be studied before such an extensive inroad is attempted into Dynamics. But, presuming that such knowledge is not possessed by the reader, we find the author performing such an integration, for example, as that of $\frac{dx}{x}$ between specified limits by a process which must strike an intelligent student as at once most ingenious and most unnatural. Special devices exhibited "for this occasion only" confer no independent power on the student, and, moreover, require him to possess an amount of ability which would be much better and more successfully employed in acquiring a knowledge of the principles of that Calculus which is thus evaded by artifice.

In all such cases it may be said, I think, that a student who is capable of understanding the notions involved will arrive at the results by other than the special artificial methods employed by the author; while in the case of a student for whom such methods are necessary, most probably the notions are unsuitable, and the method of proof is apt to puzzle him with what seems to be mathematical jugglery.

If more time were spent in teaching the mathematical principles on which quantitative physics depends, there would be less need for such methods, and in the long run the student of physics would be a gainer.

But, while advocating a more thorough and leisurely study of the elements of pure mathematics before the study of physics, it seems to me that many of our elementary text-books in mathematical physics make the mistake of occupying the student's attention with questions which, being far removed from physical reality, and being, in fact, merely disguised mathematics, had much better be omitted. We should make the effort to make our works on physics as physical as possible, to use Mathematics for the sake of Physics, and not *vice versâ*; and it is time to recognize that the field of physics is now so extensive as to supply subject-matter for calculation and for illustration of mathematical principles, and to permit us to curtail greatly the space which is now devoted to things comparatively useless.

Let me take one or two examples. Is anything gained by teaching students the very numerous properties of the curve which a particle would describe if it were projected with any velocity, under the influence of gravity, if the Earth had no atmosphere?

Again, let us turn to any work on Hydrostatics, and we are certain to find a very large number of mathematical trivialities. I say nothing of a number of liquids, whose densities are in some kind of progression, superposed in very fine tubes of peculiar shapes. These are harmless; but what about that notion of *whole pressure* on a curved surface—the sum of all the normal pressures, or, more strictly, the surface-integral of pressure over the curved surface?

Numerous, indeed, are the mathematical problems to which this notion gives rise; but what about the physical idea involved? It is easy to comprehend the trouble which a teacher lays in store for himself if he practises his students in the process of adding together the magnitudes of a number of forces whose lines of action are in all directions in space. And remember that, in most cases, very few students who are taught to calculate the whole pressure of a fluid on a curved surface will subsequently learn that a similar process has a physical meaning only in the case of the normal flux of Newtonian gravitation through a curved surface. Let us see, however, the physical meaning which is actually attributed to the whole pressure of a curved surface by the author of one of our very elementary text-books for students. The following is the literal statement:—"It should be observed that these pressures act in different directions, the pressure at each point being perpendicular to the surface at that point. The whole pressure is the sum of all these pressures, and represents the total strain to which the vessel containing the fluid, or the body immersed, is exposed." Now in this statement, besides the addition of the magnitudes of a number of non-co-planar forces, we have the misuse of the term *strain* and the completely unintelligible expression "total strain" of the vessel. And, in illustration of this definition, we are given the following example: "Show that if a sphere or a cube be filled with liquid, the total strain to which it is subjected is three times the weight of the liquid it contains." You will, I hope, agree with me in holding that such teaching is in the highest degree erroneous and objectionable, and that the efforts of those who have to teach should be directed against it.

So far as my observation goes, the principle making Physics a mere disguise—and a very unskilful one—for pure mathematics is much too largely adopted by writers on mathematical physics, with the result that many students receive very insufficient practice in acquiring and developing physical conceptions.

To this cause, also, must be attributed the fact that proficiency in mathematical physics is a much rarer thing than proficiency in pure mathematics, the former being made, to such a great extent, subservient to the latter. Indeed it was not, I think, until the publication of Thomson and Tait's "Natural Philosophy" that the ancient problem-grinding system, which hindered the progress of exact Physics, received a severe blow, and Cambridge, under the influence of the ideas and methods of Thomson and Clerk Maxwell, produced a plentiful supply of physicists.

But the good that was effected by Thomson and Tait is confined almost entirely to advanced students; in the hands of those of a lower standard are still to be found text-books of the old sort, teaching Mathematics under the guise of Physics, presenting nothing but the dry husks of the latter, and, by inaccurate language, laying an early foundation of erroneous notions. Some people of the older school—not a few—express, indeed, a strong impatience with us if we protest against the use of slipshod and inaccurate language in our text-books. They tell us that we are doing the matter of nomenclature to death, and that, after all, we need not be very particular about the choice of exact terms. This is a most unfortunate attitude; it is one, however, which this Association has done, and is doing, something to counteract. Surely it must be admitted that if the conceptions of Physics are presented to the beginner in erroneous language, there is a danger that in many instances these conceptions will never be properly acquired. And is not accurate language as cheap as inaccurate?

I know of several text-books still in the hands of schoolboys and others in which, in whole pages of answers to problems on the motion of a particle, *velocity* is always spoken of as so many feet, and *acceleration* also as so many feet—no reference being made to *time*, and the fact that acceleration has a double reference to time being never mentioned. I assume it as evident that the progress of such students is seriously hindered and delayed by such teaching.

If the writers of such text-books as those to which I refer have really clear and correct notions themselves, they ought to take pains to present them accurately to beginners who have to learn from them. Not to do so is to ignore the fact that a great deal of the difficulty of learning any branch of science is removed if only the student is started upon it with clear and correct notions as to the various entities with which the science deals, and with its fundamental principles expressed in accurate language.

Let us take, as another example of exceedingly erroneous teaching, the following exercise—one of a great many, all similarly expressed—from a text-book still in the hands of beginners: "The time occupied by a body in describing uniformly a complete revolution in a circle, whose radius is 11 feet, is 16 seconds, calculate the centrifugal force which acts upon it. *Ans.* 1·696 feet per second."

In this we have two bewildering errors instilled into the student's mind, viz. that *force* is measured in feet per second, and that when a body is revolving in a curve, it is acted upon by an *outward* normal force—the old time-honoured fallacy about centrifugal force. It is almost miraculous that any accurate scientific knowledge is acquired in spite of such fallacious teaching as this.

I wish to emphasize this teaching about *centrifugal force*, because, no matter how often it is condemned and shown to be erroneous, it still flourishes in our scientific text-books. In another elementary work which is extensively used, we find the nature of centrifugal force more fully explained in the following manner. "In order to keep a body moving in a circle, there must be a force acting upon it which will produce a constant acceleration towards the centre, equal to $\frac{v^2}{r}$. Hence, if W be the weight of the body, P the pressure tending towards the centre, $P = W\frac{v^2}{gr}$." This is, of course, all right; but then follows the mysterious statement, "a pressure equal and opposite to P is sometimes spoken of as the centrifugal force."

I call this a mysterious statement, because it does not tell us on what this pressure equal and opposite to P acts, or whether it really acts on anything at all or not. We know, of course, that this reversed force is the reaction of the moving particle on the agent or surrounding medium; but how is a beginner to know this? Will he not naturally fall into the error of supposing that it acts on the moving body itself? Or, if he possessed a little intelligence, would he not ask, "Why do you require me to think of a force equal *and opposite* to one which acts on the body? I have nothing to do, when considering the motion of the body, with any forces except those which act on the body itself; and why do you not ask me to consider a force equal and opposite to some of the other forces acting on the body—a force equal and opposite to its weight, for instance? It would be just as sensible to do so, since this reversed weight is the body's reaction against the Earth."

The only true reply to such a student is to confess that it is quite a mistake to introduce the conception at all; but instead of this, we find that the discussion proceeds to encourage the erroneous notion that centrifugal force acts on the moving body by taking the case of a carriage describing a curve of small radius.

Now, what is it that has given rise to this fallacy about centrifugal force? Is it the fact, so often adduced, that, if we tie a stone to one end of a string and, holding the other end in the hand, whirl it round, we feel an outward force pulling the hand? But then, if we imagine the stone to be attached to an elastic string, one end of which is tied to the hand, while the stone is projected vertically upwards, the hand would experience an *upward* pull; and are we thence to conclude that the stone is continually acted upon by an *upward* force?

Possibly the whole fallacy is traceable to D'Alembert, who gave us a sort of dynamical *memoria technica*, usually expressed in the words, "forces equal and opposite to the effective moving forces of a material system are in equilibrium with the external forces,"—a very imperfect and misleading statement for the principle obviously contained in Newton's first and second axioms, viz., "the mass-accelerations of the particles of any material system have at each instant *the same* total component along any line, and *the same* total moment round any axis as the external forces acting on the system."

D'Alembert's fictitious reversal is unnatural and unnecessary; and, in introducing fictitious forces and reducing the state of the moving material system to one of equilibrium, he ignored the actual state of affairs. He should have faced the state of motion as it exists, and recognized the fact that the production and maintaining of a given state of motion requires the action of a definite system of forces assisting the motion, instead of concentrating his attention on something which would stop the motion. It is this determination to fix the mind on a state of equilibrium, and to ignore the actual motional state that is, I think, responsible for the fallacy of centrifugal force.

[Other examples of erroneous teaching with regard to centrifugal force, the confusion of *work* with *horse-power*, &c., were then given.]

We must remember, also, that a teacher who has often to correct the erroneous language of the text-book which he employs with his class is by no means certain of inspiring confidence in himself and convincing his pupils that the teaching of the author is at fault; for, young students, very unlike Prince Bismarck, have a profound respect for printer's ink; hence they are always disposed to believe that the book is right, and if you tell them that an acceleration is not so many *feet*—as the book says it is—but so many *feet per second per second*, they accept the correction for politeness or for peace sake, but not with any conviction.

In addition to numerical calculation as an important aid in the teaching of Mathematical Physics, I would also advocate the employment of *graphic methods of solution* in cases in which exact solution is not possible. The graphic method of solution is scarcely recognized at all in our text-books. Every text-book of Statics gives us, indeed, its modicum of graphic representation, which is usually exhausted in a bare and barren exposition of the principle of the *triangle of forces* and the *polygon of forces*, which has long since become stale "book-work."

But this is by no means what I advocate. I intend graphic solution at once as an aid to calculation, as a means of strengthening the student's interest in the subject, and as a representation to the eye of the possibilities and limitations of any particular problem under discussion.

[The problem of the equilibrium of a camp-stool was then given as an illustration of the graphic method.]

In Statics and Hydrostatics we have abundant instances of this kind, and their solutions by the graphic method furnish excellent practice for the ingenuity of the student, giving to each problem the interest which a vivid picture always gives, as well as strengthening his knowledge of the results and methods of pure Mathematics.

The sum total, then, of what I have said with reference to our elementary text-books comes to this—that, while we are abundantly supplied with high-class scientific works of an advanced character, a corresponding improvement has not generally taken place in the books from which school-boys and other beginners have to learn; that these books are often marred, not only by inaccurate language and fallacious teaching, but by a certain scrappiness in their mode of treatment which is encouraged by the desire of examining bodies to hurry through and skim over a large number of scientific subjects at the expense of a more leisurely study of the foundations of Science.

I should like, in conclusion, to say a few words with regard to the way in which the cramming of Science is encouraged by examinations.

Wherever well-worn book-work is set at an Examination, it is extremely likely that cram will find its opportunity, and it may be impossible for an examiner to detect it. Assuming that book-work must be set, I do not know of any reliable safeguard against cramming except *vivâ voce* examination. But a *vivâ voce* examination which is made a part of the competition, and in which the candidates do not all get the same questions, is, I think, essentially unfair. Whenever such an examination is possible, its function should be to discuss with each candidate the several questions with which the written paper deals. In this way, when his knowledge of any particular question or method appears to the examiner of the written paper to be doubtful, the doubt is very speedily settled one way or the other. But, in the absence of such a corrective, an examiner who is, after all, dissatisfied with the candidate's working of any particular question, and yet disinclined to allow him no credit whatever, is obliged to resort (at least in many instances) to some systematic method of cutting off marks, and this method is not infrequently a system of elaborate trifling.

As a particular example, take a question which is sometimes set at examinations in Science—the method of determining H, the horizontal intensity of the Earth's magnetic force. A part of the process consists in the discussion of the vibrations of a uniform magnet bar round a vertical axis under the influence of the horizontal magnetic force. Such a bar is a compound pendulum, and its motion involves a knowledge of its moment of inertia and the integration of a differential equation of the second order. If at any instant θ is the angle made by the axis of the bar with the magnetic meridian, the expression $\frac{d^2\theta}{dt^2}$ is involved in the equation.

Now, I know a case in which the whole of this process was crammed up by a candidate who, although he had a good practical knowledge of Physics, did not know how to resolve a force along a line, did not know the meaning of the moment of a force about an axis or of the moment of inertia of a body about an axis, and, of course, was wholly ignorant of the meaning of the expression $\frac{d^2\theta}{dt^2}$. Indeed, this last was a puzzle to him, for he always failed to place the two figures "2" in their proper places. Yet the whole thing was crammed up with sufficient success to obtain credit for the question.

In such a case as this an examiner may have very grave suspicions, but he may be obliged to give a large measure of credit, nevertheless. Frequently the method of marking such an answer is something like this—the expression $\frac{d^2\theta}{dt^2}$ involves two d's, two 2's, a t, and a θ; if the candidate uses all these symbols, each in its proper place, give him full marks; if he uses all the symbols, but *one* is misplaced, take off *one* mark; if *two* are misplaced, take off *two* marks; and so on. This is of course an exaggeration, but it is the *kind* of system adopted—necessary, perhaps, but extremely unsatisfactory.

Now a *vivâ voce* examination of the candidate on this question would, in a few seconds, have decided his mark to be *zero*.

In cases in which the number of candidates is very large, it would seem to be impossible to apply a *vivâ voce* test; but there are cases in which it could be used, and used, I have no doubt, with good effect.

The setting of book-work at examinations is, I think, much too frequent. It gives a hope of success to candidates who really have no vocation for a subject, but who load their memories sometimes with whole pages of work of which they understand almost nothing.

Our examination system has its defects; but these defects are almost wholly due to the shortcomings either of individual examiners or of the directors of education.

I have spoken of the excessive haste with which educating bodies compel students to attempt the acquisition of a large and varied assortment of knowledge; but I must not omit to add that the evils of examination are greatly aggravated by the prodigious haste which is imposed on examiners themselves in some public competitions. Nothing can justify a system which requires an examiner to read over and determine the numerical value of every one of 1200 papers in little more than a fortnight. The thing is utterly impossible, and the results must be untrustworthy.

But, whatever the defects of the examination system may be, I hold that the *principle* of examination is good.

[Some of the objections of the writers in the *Nineteenth Century* were then dealt with in detail. Referring to the unsatisfactory method by which professors and lecturers are now appointed, Prof. Minchin continued:—]

Mr. Frederic Harrison says, "Trust the teacher; trust him to teach, trust him to examine;" and I should say that, if we did, we should find our trust far more often misplaced than it would be by trusting wholly to the independent examiner. If the teachers were always people who could teach, the case would be different; but what is the method of appointing teachers, as a rule, in England? People make inquiries as to what scholarships, prizes, and degrees have been obtained at the University by a candidate for a teaching post; but of his capacity for imparting successfully any of his own knowledge to others there does not, in most instances, exist the slightest proof; and hence we are often presented with such lamentable teaching failures. Under these circumstances it would be ridiculous to "trust the teacher, trust him to teach, trust him to examine;" for, bad as examiners are, teachers are worse—partly for the same reason as that for which white sheep eat more than black, viz. that there are more of them.

And is it not also vain to attempt to replace the Examination system, even at the Universities, by a system of Thesis writing on some particular subject selected by the candidate, the Thesis being written by him at home, with ample opportunity for composing it out of works of reference, or with the assistance of his friends, with the addition of a small make-believe of "original research?"

We are told by one of the writers in the *Nineteenth Century* that, under our Examination system, England is losing her intellectual giants, men who are a head and shoulders above their contemporaries; but I ask is the writer aware that we have still among us Sir William Thomson, and that we have only recently lost Clerk Maxwell? People who speak thus seem to think that we are to expect a crop of intellectual giants to sprout up like a crop of mushrooms, and to be ready whenever we want them.

I would say in conclusion—let us retain Examinations, make them tests of a knowledge of the applications of principles, so that they will be real tests of intelligence, and not of cram; do not make them so difficult and so high-flown as they often are; and, finally, do not encourage the hurrying and skimming through a large number of different subjects, but make sure of the foundations by a more thorough and leisurely system of study.

THE LIFE-HISTORY OF A MARINE FOOD-FISH.[1]

I.

IT is but a few years since the life-history of our most important marine food-fishes was involved in considerable obscurity—not only as regards popular views, but even in respect to the knowledge of men of science. Thus, for instance, in the years 1883 and 1884 the almost unanimous opinion of British fishermen was that our common food-fishes sought the shallow water of the bays and inshore ground generally for the purpose of depositing their eggs on the bottom. No observations specially bearing on this point had been made by British zoologists, and a series had to be undertaken for a public inquiry then in progress—with a result which demonstrated how extensive the reverse of the popular notion was. Again, certain comparatively recent authors on British fishes speak of a common fish like the gurnard as spawning twice a year, whereas, after careful observation, no evidence in support of this view has been obtained. The same obscurity veiled the larval and post-larval conditions of most of the food-fishes, even G. O. Sars—in regard to the latter stage—describing no intermediate forms between the larva of 6 mm. and the post-larval stage of 24 mm. in the cod, almost the only fish to which some attention had been paid.

On the other hand, our knowledge of the development and life-history of the fresh-water fishes—such as the salmon, trout, and charr—has for many years been well understood—thanks to the labours of Louis Agassiz and Vogt in Switzerland, Coste and Lereboullet in France, Ransom in England, and Shaw in Scotland, on the scientific side, and of the noblemen and gentlemen of Perthshire (ably seconded by Robert Buist) in connection with Stormont Field Ponds on the Tay, on the popular side. Much information has also been recently obtained by Dr. Day and Sir J. Gibson Maitland at the excellent ponds of the latter at Howietoun.

A short time ago, relying on experience derived from fresh-water fishes, not a few imagined the eggs of marine fishes as readily visible and tangible objects—possibly associated in their minds with certain practices in trout-fishing, or it may be with the manufacture of caviare. Recent investigations, however, have shown that in most marine food-fishes the eggs are minute glassy spheres which float freely in the ocean. For a knowledge of this fact we are indebted in the first instance to Prof. G. O. Sars, of Christiania, a naturalist trained from boyhood under a distinguished father, and who, by a fortunate appointment to a fishery post in Norway, was enabled to discover that the eggs of the cod, haddock, and gurnard, floated in the water, or, as we term it, were pelagic. He thus opened up a new field in the economy of the food-fishes, which in a great maritime country like ours ought not to have remained so long unexplored.

Lately, however, attention has been earnestly directed to the subject, and the labours of Cunningham, Brook, Prince, and others have made considerable advances in this department.

It is now known that the great majority of our British marine food-fishes—indeed, all our most valuable kinds (including even the sprat and the pilchard amongst the Clupeoids) produce minute eggs—as transparent as crystal, and which float freely throughout the water. These eggs, moreover, are not all shed at once, as in the case of the salmon, but successive portions of the ovary become ripe, and the eggs then issue externally. If by any accident or irregularity—as for instance the confinement of a flounder in an unhealthy tank—this gradational issue is interfered with, the animal dies from the great distention of the body caused by the pent-up eggs. In the case of the cod this gradual issue of the eggs continues probably for a week or two, so that the progeny of a single fish in one season may vary considerably in size.

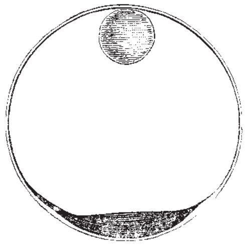

Fig. 1.[1]—Pelagic egg of the ling (enlarged).

From the early months of the year onward to late autumn the sea off our shores thus abounds with pelagic eggs, those of the rockling, haddock, and sprat being amongst the earlier forms, while the later include those of the sole. As indicated in the Trawling Report, and now supported by further experience, it would be a very difficult matter indeed to arrange for a close time in the sea—that is to say, for a limited period during which the mature fishes might be permitted to spawn in peace. This, however, in the case of individual species, such as the cod, might more readily be carried out, so as to save the mature fishes at the spawning period.

In a vessel of still sea-water these transparent glassy spheres rise at once on issuing from the fish, and form a stratum on the surface. Even the ripe portions of ovaries removed from the rejected viscera on a pier will show the same features, and thus, indeed, they first came before the lamented Lord Dalhousie at Anstruther. In the sea, however, they are seldom met with on the surface, and the tow-nets require to be sunk a fathom or two for their capture, their specific gravity being so little less than that of sea-water that they are carried hither and thither by the

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[1] A Discourse delivered by Prof. W. C. McIntosh, F.R.S., at the Royal Institution, on Friday, February 1, 1889.

[1] I am indebted to Mr. E. E. Prince, B.A., for kindly aiding me with sketches for the woodcuts. The sketch of *Motella* is by Dr. Scharff.